

PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES

Volume 3

FEBRUARY 15, 1917

Number 2

ATLANTIS AND THE PERMANENCY OF THE NORTH ATLANTIC OCEAN BOTTOM

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Communicated, January 8, 1917

In 1912 Prof. Pierre Termier, the director of the Geological Survey of France, delivered before the Oceanographic Institute of Paris a very interesting and stimulating lecture on the probable existence of Plato's Atlantis. This lecture is now published in English in the *Annual Report of the Smithsonian Institution* for 1915 (1916), pages 219-234. In his lecture the speaker drew a conclusion that needs to be examined, as it is of considerable importance in paleogeography whether one is in harmony with it or not. Termier thinks it "A fair conclusion . . . that the entire region north of the Azores and perhaps the very region of the Azores, of which they may be only the visible ruins, was very recently submerged." This means that the area believed to have been submerged is at least equal to 40,000 square miles, and may be even far more than 200,000 square miles; it is said to have sunk quickly about 10,000 feet beneath the surface of the sea.

What are the facts that lead Termier to this very important conclusion? He relates them as follows:

Some cataclysms certainly have occurred, and they date only as from yesterday. I ask all those who are concerned with the problem of Atlantis to listen attentively and to impress on their mind this brief history; there is none more significant: In the summer of 1898 a ship was employed in the laying of the submarine telegraphic cable which binds Brest to Cape Cod. The cable had been broken, and they were trying to fish it up again by means of grappling irons. It was in north latitude 47° 0' and longitude 29° 41' west from Paris, at a point about 500 miles north of the Azores. The mean depth was pretty nearly 1700 fathoms, or 3100 meters. The relaying of the

cable presented great difficulties, and for several days it was necessary to drag the grappling irons over the bottom. This was established: The bottom of the sea in those parts presents the characteristics of a mountainous country, with high summits, steep slopes, and deep valleys. The summits are rocky, and there are oozes only in the hollows of the valleys. The grappling iron, in following this much-disturbed surface, was constantly being caught in the rocks by hard points and sharp edges; it came up almost always broken or twisted, and the broken pieces recovered bore large coarse striæ and traces of violent and rapid wear. On several returns, they found between the teeth of the grappling iron little mineral splinters, having the appearance of recently broken chips. All these fragments belonged to the same class of rocks. The unanimous opinion of the engineers who were present at the dredging was that the chips in question had been detached from a bare rock, an actual outcropping, sharp-edged and angular. The region whence the chips came was furthermore precisely that where the soundings had revealed the highest submarine summits and the almost complete absence of oozes. The fragments, thus torn from the rocky outcrops of the bottom of the Atlantic, are of a vitreous lava, having the chemical composition of the basalts and called *tachylite* by the petrographers. We are preserving some of these precious fragments at the Musée de l'École des Mines at Paris.

The matter was described in 1899 to the Académie des Sciences. Few geologists then comprehended its very great import. Such a lava, entirely vitreous, comparable to certain basaltic stones of the volcanoes on the Hawaiian Islands, could solidify into this condition only under atmospheric pressure. Under several atmospheres, and more especially under 3000 meters of water, it might have crystallized. It would appear to us as formed of confused crystals, instead of being composed solely of colloidal matter. The most recent studies on this subject leave no doubt, and I will content myself with recalling the observation of M. Lacroix on the lavas of Mount Pelée of Martinique: Vitreous, when they congealed in the open air, these lavas became filled with crystals as soon as they were cooled under a cover, even not very thick, of previously solidified rocks. The surface which today constitutes the bottom of the Atlantic, 900 kilometers (562.5 miles) north of the Azores, was therefore covered with lava flows while it was still emerged. Consequently, it has been buried, descending 3000 meters; and since the surface of the rocks has there preserved its distorted aspect, its rugged roughnesses, the sharp edges of the very recent lava flows, it must be that the caving in followed very close upon the emission of the lavas, and that this collapse was sudden. Otherwise atmospheric erosion and marine abrasion would have leveled the inequalities and planed down the entire surface. Let us continue our reasoning. We are here on the line which joins Iceland to the Azores, in the midst of the Atlantic volcanic zone, in the midst of the zone of mobility, of instability, and present volcanism. It would seem to be a fair conclusion,

then, that the entire region north of the Azores and perhaps the very region of the Azores, of which they may be only the visible ruins, was very recently submerged, probably during the epoch which the geologists call the present because it is so recent, and which for us, the living beings of today, is the same as yesterday.

Now let us see what are the geologic conditions of the Azores. Gagel¹ tells us that this group of nine islands rises out of the Atlantic from depths of 10,945 feet to elevations of 3250 feet, and in one case even to 8040 feet above the surface of the sea. There are here no old sedimentary or old eruptive formations and the islands appear to be of very recent volcanic origin. Among the volcanic materials have been found only inclusions of fossiliferous middle Miocene limestone. He concludes that they are volcanic islands of Tertiary age that are made up in the main of trachytic and basaltic lavas, that these have probably built themselves up to elevations of from 16,250 to over 21,125 feet, and that some of these volcanoes have been active during the past four centuries.

If the region of the Azores and that to the north of them for many hundreds of miles had been parts of a great continent now sunk deep into the Atlantic, there should be some evidence of this sinking shown in a well marked elevated sea terrace all along the Atlantic, for it is postulated that Atlantis sank when humanity had attained a high state of civilization; in fact the time when the warriors, according to Plato, came from Atlantis cannot be more ancient than Egyptian history. In other words, Plato's Atlantis must have disappeared not more than 8000 to 10,000 years ago, for the priests of Egypt told "of a singularly powerful army, an army which came from the Atlantic and which had the effrontery to invade Europe and Asia Later, with great earthquakes and inundations, in a single day and one fatal night, all who had been warriors against you [Athens] were swallowed up. The island of Atlantis disappeared beneath the sea."

The area of land supposed by Termier to have sunk is not less than 40,000 square miles, and if we accept his greater supposition that it included also the region to the north, the mass would be not less than 700 miles long by 300 miles wide, or 210,000 square miles. Now let us see how much water the sinking of such masses is equal to, with a view of learning how much the eustatic strand-line of all oceans would be lowered. Murray estimates the superficial area of the oceans as about 139,000,000 square miles, and the mean depth as 13,000 feet. Therefore to sink a mass so small as 40,000 square miles to a depth of 10,000 feet would only lower the general strand-line a little more than

3 feet. If, however, the greater estimate of 210,000 square miles be taken, then the oceanic level would be reduced about 15 feet and this should show in a well marked terrace all along the Atlantic shores. However, it is not only in the Azores that Termier seeks for Plato's lost land, but in the Canary and Cape Verde Islands as well. In other words, he believes that a continent greater than anything assumed for the Azores has very recently foundered, and therefore we should all the more easily observe an elevated strand-line along the Atlantic shores of North America. It is true that there are at least three Pleistocene elevated terraces recorded in Maryland, the highest and oldest one at 220 feet, known as the Talbot terrace, the middle Wicomico one at 100 feet, and the lowest and youngest at 40 feet, the Sunderland terrace. None of these, however, can have any connection with the foundering of Atlantis, as they are far older in age than Plato's account. On the other hand, these and the other Pleistocene terraces are due not only to isostatic and orogenic factors, but also to the climatic factor, as explained by Barrell.² From this we see that if a continent situated in the Atlantic foundered into the depths of this ocean, it must have done so in far more ancient times than those of civilized man. Furthermore, the geology of the Azores shows that these islands are not parts of a foundered continent, but that they are volcanic islands that have arisen above the Atlantic bottom during the latter part of Cenozoic time. On the other hand, we learn from Gagel that five of the islands of the Cape Verde group and three of the Canaries have rocks that are unmistakably like those common to the continents. Taking into consideration also the living plants and animals of these islands, many of which are of European-Mediterranean affinities of late Tertiary time, we see that the evidence appears to indicate clearly that the Cape Verde and Canary Islands are fragments of a greater Africa. It is therefore not to the north of the Pillars of Hercules that we should look for Atlantis, but to the southwest of the rock of Gibraltar.

To follow out another line of evidence, the writer understands that petrographers know little from actual observations as to the behavior of flowing lavas under the sea, and whether the cooling phenomena and the formation of vitreous lavas would be the same there as beneath the atmosphere. At least three of them, however, are of the opinion that tachylyte would form equally as readily beneath the sea as on land. In answer to a request for data that might bear on this problem, Doctor A. L. Day, director of the Geophysical Laboratory of the Carnegie Institution of Washington, directed my attention to a recently published paper by Perret.³ Last year the latter studied the

flow of lava descending from the volcano of Stromboli, in Sicily, and entering the sea, and in 1914 a similar occurrence at Sakurashima, Japan. At Stromboli a surface flow was 20 meters wide, and the hot lava "entered the water at an average rate of about 3 cm. per minute." A little distance beyond the actual contact with the water and in "a perfectly calm sea there is rarely a continuous and uniform evolution of vapor. At Sakurashima, on March 12, 1914, the lava, at one place, was entering a sea as smooth as glass, yet the evolution of steam was spasmodic and resulted in a series of puffs." In regard to the subsurface flow of lava, a condition of greater importance in the present discussion, Perret writes as follows:

At Sakurashima there was a submarine lava flow extending from beneath the eastern lava field for a distance of 2 kilometers along the sea bottom. The lava had a depth of some 75 meters, with 40 meters of water above it The only disturbance visible at the surface was a succession of convection currents in the water, without eruption of gas, and without raising the water temperature above 64° F. at the surface and 72° just over the lava.

He concludes "that a flowing lava may exist in contact with water without the disintegration of either, thanks to the formation of a protective sheath, and this fact helps us to understand the quiet growth of submarine volcanoes. In such cases the only surface commotion need be that due to true gas emission at the central vent. In point of fact, a sub-aqueous lava stream comports itself more decorously than a similar sub-aerial one." This is due to an important fact, namely that even if the protective cooled sheath is broken in places "and a little water enter and be vaporized in the act of sheathing the raw places that which is thus evolved is simply the vapor of water, and this, in the presence of water in mass, condenses to water again—there is nothing to reach the surface and cause ebullition."

Doctor Day in the letter to me mentioned above, dated November 27, 1916, comments on Termier's conclusion and the observations of Perret as follows:

I have just read Professor Termier's interesting speculation entitled 'Atlantis' and must confess that I find nothing in my experience with which to support his views. In the forthcoming number of the *American Journal of Science* you will find an article by Perret [the one reviewed above] who is one of the most accurate observers of volcano phenomena with whom I ever came in contact. In this article he records in unmistakable terms that there is no essential difference in the behavior of a sub-aqueous and a sub-aerial

flow other than that which may be exerted by the superimposed hydrostatic pressure. From such experience as we have gathered in this laboratory, hydrostatic pressure can have no other effect than to raise the melting temperature 10 or 20° per thousand atmospheres, that is, 1 or 2%, and this factor must therefore be accounted comparatively insignificant in determining solidification. It is conceivable that great hydrostatic pressure might have the effect of preventing the escape of the volatile ingredients contained in a sub-aqueous lava flow and so facilitate crystallization. It is our experience that a very small quantity of such ingredients has enormously greater influence in determining crystallization than a very large hydrostatic pressure alone. It might therefore follow that the pressure operating in this indirect manner might serve to keep volatile ingredients 'on the job,' so to speak, which would otherwise escape and so promote crystallization in a mixture which would otherwise tend to cool in vitreous form. Beyond this possibility I can conceive of no basis in present experience for the assertion which Termier has made. In general, silicate mixtures which crystallize with difficulty will form glass if cooled quickly whether under pressure or not—the pressure apparently being the least important factor in the situation. Similar mixtures which crystallize readily can with great difficulty be cooled quickly enough to prevent crystallization, and here again the factor of pressure is relatively insignificant.

You may recall a paper by Johnston two or three years ago in which he showed plainly and unmistakably that in general small changes of temperature or concentration would have greater effect in determining the resulting solid form than a thousand atmospheres of pressure. This conclusion is in a sense obvious, for if a thousand atmospheres will produce no more than 10 or 20° effect on the melting temperature, then obviously 10 or 20° temperature change in this temperature region will be its equivalent. In the same sense a 1 or 2% admixture of one of the volatile ingredients will produce several tens of degrees lowering of the melting point of the solution in this temperature region. These considerations are perfectly general and apply without reservation to the condition of things which Termier is discussing. I am therefore disinclined to give any weight to the evidence which he adduces in proof of the contention that vitreous basalts could not have formed at depth as well as anywhere.

This paper has also been read by Prof. L. V. Pirsson, and he makes the following comments in regard to the formation of tachylytes:

Whether a magma will solidify in a vitreous or a crystalline condition appears to be much more due to temperature than to pressure. The latter, in the quantities which we have to deal with in the superficial crust of the earth, seems relatively negligible compared with very moderate changes of temperature. If the change of temperature of a basaltic magma on attaining a sub-aerial surface is sufficient to cause it to solidify as a glass, or tachylyte,

as we know it may, there seems no good reason why a basalt magma issuing into cold water on the sea-floor might not be similarly affected and have an upper glassy crust. Such a glassy skin on the lava would seem an even more natural result from the melt being plunged into cold water than if it cooled in the air, the pressure of the depth of water being a minor consideration compared with the sudden change of temperature. The experience of mankind from remote ages has taught that the quickest and most convenient way of suddenly cooling a heated material is to plunge it into cold water.

That basaltic glasses, or tachylytes, are not formed solely under atmospheric conditions is shown also by the fact that they have been found as the selvage edges of intrusive rocks, in dikes, and in intrusive sheets, in Finland, Sweden, Connecticut, and elsewhere. These glassy sulbands are now revealed to us only after prolonged erosion, and the geologic evidence would appear to indicate that they were probably formed under greater pressures than would be produced by the weight of the water of the ocean. It was the sudden chilling, produced by the contact with cold rocks, which forced the glass to form in spite of the pressure.

In the light of petrographic experience it does not seem that the generalization of Professor Termier is well founded. The fact of dredging glass splinters from oceanic depths in a volcanic region can hardly be held in itself as a proof of profound subsidence of such an area from sub-aerial conditions.

The conclusions from these various studies are (1) that the Azores are volcanic islands and are not the remnants of a more or less large continental mass, for they are not composed of rocks seen on the continents; (2) that the tachylytes dredged up from the Atlantic to the north of the Azores were in all probability formed where they are now, at the bottom of the ocean; and (3) that there are no known geologic data that prove or even help to prove the existence of Plato's Atlantis in historic times.

The greater question, was Africa ever united to South America is being answered by biologists and geologists, 'yes' and 'no.' The writer, however, believes in this connection previous to the Tertiary and thinks that the down-breaking of western Gondwana began in the late Lower Cretaceous, with complete severance long before the close of Eocene time, for marine strata of this age are general along the western border of Africa. On the other hand, if this land bridge had continued unbroken into Tertiary time, even only as late as the later Eocene, then certainly the wonderful fossil mammalian faunas of Argentina should have revealed many and unmistakable African links. The African affinities in the ancient South American mammalian faunas are, however, so slight as to give but a very limited support to the theory that Gondwana was still in existence in early Tertiary time, and none at all to

the theory that the South Atlantic bridge was present even in the Miocene.

¹ Gage, C., *Handbuch der Regionalen Geologie*, 7, Pt. 10, Heidelberg, 1910.

² Barrell, J., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), 40, 1915, (1-22). Also see Wright, W. B., *The Quaternary Ice Age*, London, 1914, chaps. 16 and 18; and Goldthwait, J. W., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), 32, 1911, (291-317).

³ Perret, F. A., *Amer. J. Sci., New Haven, Conn.*, (Ser. 4), 42, 1916, (443-463).

THE RESPONSES OF HYDROIDS TO GRAVITY

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Read before the Academy, November 14, 1916

Corymorpha palma is a solitary hydroid occurring on the mud-flats of False Bay and other like localities in the neighborhood of La Jolla, southern California. It has a length of body that may exceed even 6 or 7 cm. In its natural position under water its basal end is imbedded in the mud, above which its stem rises vertically, carrying at the opposite end the somewhat drooping head. When *Corymorpha* is removed from the mud and allowed to attach itself to some foreign base which can be conveniently turned in an aquarium, it assumes in a very short time a vertical attitude irrespective of the position of the base. This vertical attitude is acquired and maintained by the stem even after the head has been cut off and this part may therefore be said to exhibit negative geotropism. The stem is provided with a neuromuscular sheath and a core of vacuolated cells like those in the chorda of vertebrates. Torrey has raised the question of the relative importance of the neuromuscular sheath and of the core cells in bringing about the geotropic response and has advanced evidence in favor of the view that the core cells, acting as certain vegetable tissues often do, are the mechanism of this response. If, however, *Corymorpha* is placed in sea-water containing some chloretone, by which the neuromuscular activity is abolished but the core cells are left unchanged, no geotropic response can be obtained when the animal is moved out of the vertical. When the core cells are disorganized by twirling a needle in the axis of the stem, care being taken that the neuromuscular sheath is not injured, the stem will show a tardy but successful geotropism. It therefore seems probable that the geotropic response in *Corymorpha*, as in most other animals, is the result of the activity of the neuromuscular sheath and not of the core cells, though the latter very probably help to maintain the geotropic position by assuming a somewhat fixed